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THE EARLY SURROUNDINGS OF LIFE[1]

THE American Association in its Plattsburg meeting is close to the shore lines of the first ocean that seems to have contained organic life in variety, or rather a life that had such hard parts that a tolerably complete record of the main groups and families has come down to us. It is then natural to consider what the conditions may have been under which this so varied and complex life had developed, without leaving more trace of its existence.

Reading over Darwin's "Origin of Species," one can readily see that of all the objections to his theory which he so fully and fairly presented, that which he deemed the most serious was the lack of connecting links in the geological record, and in particular the sudden appearance of the varied primordial life.

He conceded that this latter objection was valid so far as one then knew, and ventured only to suggest that while the continents and oceans had been in grand outline fairly permanent since early Paleozoic, during longer eons previous, which he felt must have elapsed, conditions might have been reversed, and the sediments then laid down have been buried beneath the oceans or altered with their life beyond recognition.

At about the same time that Darwin

[1] Vice-presidential address before Section E of the American Association for the Advancement of Science, also complimentary to the Catholic Summer School at the Champlain Assembly near Plattsburg, N. Y.

was pondering his theory, Logan and the New York geologists were studying and giving names to strata antedating the primordial animals and in the fifty years since not only has the theory of the creation of species by generation advocated by Darwin won practically universal scientific acceptance, but many thousand feet of rocks laid down before the Paleozoic have been much studied.[2] One might infer that discoveries in these beds had removed the difficulties. This is not true. The Darwinian theory has won acceptance by its marshaling of facts in other lines. The most serious difficulty still remains most serious. The years have indeed brought so many connecting links to light since Paleozoic time that we may reasonably expect to find more, and in view of the imperfections in our knowledge of the geological record and the fact emphasized by Rice last winter that the record itself is likely to be particularly imperfect just at the critical and exciting parts of the story of life, the lack of more such links seems no longer very serious.

On the other hand, the difficulty at the beginning has in some ways increased. Beds before the primordial, but little altered, such as later preserve ample traces of life—black slates, limestones, dolomites —show only obscure traces.

Nor does the total thickness of such beds suggest a time before the Paleozoic longer than that since. Astronomers and physicists are putting limits to the age of the earth, which though ample for the deposition of all known sediments, curtail the age of the planet as an abode for life to a number of years which the Paleozoic and later beds may easily have taken to form. At the same time the discovery of fishes in the Ordovician shows that the tree of Life had developed all its main branches at that early date. Where then and how did early life manage to do this ''90 per cent.'' of its differentiation so quickly or with so little trace of itself?

Besides the answer suggested by Darwin which seems no longer admissible—it was proposed by him with the greatest reserve —three notable suggestions have been made toward lessening the difficulty. I refer to those of Brooks, Chamberlin and Daly.

Brooks imagines that the early forms of life were free-swimming surface forms of the deep sea, not freely preserved until they discovered the shore as a habitat.

Chamberlin suggests, on the contrary, that the early life was developed in fresh water, in streams and landlocked waters. Dwellers in such locations have rarely left any trace of themselves, in the rocks. Fresh water dissolves their shells, while the deposits themselves are liable to frequent rehandling.

Daly has recently suggested that the chemical character of the water was the determining factor, and that (during Eozoic time) the ocean was limeless so that the animal could not secrete hard parts.

I will not just here go into any elaborate discussion of these theories and the arguments for and against. They are not compatible. Each has almost obvious difficulties, and it is clear that farther light will be very welcome. I wish first to call attention to a ray of light which the geologist might easily overlook, since it is due to a physiologist, one of the assistants at the Collège de France, R. Quinton. He has written a brilliant book of some 500 pages[3] to defend the thesis which he had proposed in 1897 that the higher animals show traces in the vital fluid of their original environment. Let me explain.

[2] The latest fruit being the report of the Adirondack Committee, *Jour. Geol.*, 1907, pp. 191–217.

[3] "L'Eau de Mer Milieu Organique," Masson, Paris, 1904.

For Quinton the higher animals are compound colonies of individual protoplasmic cells, made up: first, of these cells, that is, living matter or protoplasm, red blood corpuscles, phagocytes, etc.; secondly, of secreted dead matter such as coral, bone matter, muscle fiber, etc.; thirdly, of various excretions, and secretions like milk; fourthly, of the vital fluid, the blood serum, free from all corpuscles or extraneous matter, the lymph, the plasm, or "physiological salt solution" which fills the body and bathes the protoplasm, and is the universal circulating fluid.

Now Quinton states that this vital fluid, which is but the water in which they live in the lower sea animals, represents the same thing in the osmotically closed higher animals, and tends to represent the original ocean or, as he formulates it (p. 417), "Animal life, which appeared in the state of a cell under definite physical and chemical conditions tends to maintain through the evolutionary series, in spite of cosmic variations, these conditions of its origin."

Quinton accordingly compares man to a marine aquarium filled, however, not with present-day sea water, but with that of the early ocean. Again he compares him to the culture tube of a bacteriologist—the tube represented by the dead matter, the skin, etc., the culture by the living cell matter, while the vital medium represents the nutrient fluid. These are striking and stimulating comparisons. It is rather a commonplace to say that society is an organism. It is not so common to say the converse: "An organism is a society."

The great value of a scientific theory is in its capacity to marshal and correlate facts, and stimulate lines of investigation. Judging Quinton's hypothesis by this test, I feel safe in classing it as a very valuable addition to science. Quinton's premise that animal life had a marine origin will, I believe, be so readily accepted by geologists that I shall take it for granted and not even sketch the elaborate arguments, embryologic, phylogenetic, and others by which he proves it.

This granted, that there should be some tendency among animals with a closed body cavity like the land animals to retain the ancestral composition of the vital fluid seems reasonable. But Quinton goes on to maintain that it is not a mere tendency, but to a very great degree successful, and that from the composition of the vital fluid one may safely infer that the early ocean had a temperature of about 44° C. (111° F.) a concentration of about seven to eight parts per thousand, and a composition mainly of sodium chloride. Now in this we can not follow him without further consideration, and we are going to ask: first, what actions are now going on which may have changed the ocean from the physical and chemical conditions of the vital fluid to those that it now has, and, secondly, what traces are there of the burial of any such waters? For there are alternatives to Quinton's hypothesis which he hardly seems to fully realize.

That environment produces on the vital fluid some effect is beyond question, and is granted by Quinton, as it is shown by his experiments. Now if by any particular modification conditions more favorable to cell life are produced, why should not this modification be accumulated by the survival of the most vigorous, and so there be a progress from the original oceanic fluid to one determined, not by the original ocean composition, but simply by physical and chemical factors, which make it the best for cell life?

Again, why may not the sea animals have remained open so long as the ocean was growing more favorable in condition for life, and only closed themselves in or struck

out for the land when in its change it passed the optimum. We shall, I think, find that this latter hypothesis is the more likely to be true. But it does not seem that we can readily dismiss the second from consideration.

There are other weaknesses in Quinton's theory, one of which we may now mention, leaving the rest until later. That is that his theory does not take in plants, and their circulating medium.

The lower plants and animals are not far apart, and we can hardly suppose a different origin. Yet plants must have existed before animals, just as herbivora must have existed before carnivora, and the cat presupposes the existence of the mouse. Plants as well as animals have protoplasm or living matter, dead matter, and secretions and a vital fluid or sap. But the sap is of much lower concentration and different composition from animals. Quinton's hypothesis needs to be supplemented by some explanation of the relations between plants and animals.

We are then prepared to consider Quinton's valuable law with an open mind, and see how far its inferences as to the surroundings of early life agree with those of dynamic and historical geology.

1. Quinton infers that the early ocean had a temperature not far from 44° C. (111° F.) which is not far from the hottest blood temperature of birds. It is certainly a remarkable fact that from the tropics to the poles, in spite of the tendency of the environment in arctic and temperate climes, the blood heat of the more vigorous and active animals in the various orders of vertebrates falls not over 10° below this temperature.

Quinton's explanation is not the only one, however. The other is that the processes of oxidation and combustion, which furnish the energy for the bodily activity, and the supply of heat to keep the body warm, raise the same to a heat which is best for cell activity, or perhaps even to the temperature which can be stood without serious damage.

A careful investigation of blood temperatures and bodily activity, which are in some ways correlated, might be very significant.

Geologically we can not speak with as much assurance as we might have before radium and the newer theories of cosmogony had undermined all certainties. Glacial periods indeed are reported upon strong evidence from early geological times. Nevertheless, whether we believe in a gradually refrigerating climate, on a cooling globe, warmed by a dying sun, or not, the former wide extent of corals and ferns toward the north pole is undoubted. Van't Hoff finds a hot climate indicated by the Stassfurt salt deposits. We must therefore allow the possibility, and I think most of us would say probability, of a much warmer ocean at least at times in the past, than at present. We may then imagine, and it was to me an illuminating thought, that the early "fish" were not cold-blooded animals at all, but active warm-blooded creatures whose blood temperature was that of the warm ocean around, which has been retained by the higher of their descendants. It seems probable that such a warmer ocean would accelerate all organic activity, including evolution.

Upon the basis of a cooling environment Quinton builds an ingenious genealogical tree. He imagines the secular cooling of the surroundings as depressing also the body temperature of all except a few forms which make special modifications to keep it up. Then a farther fall will lower all except a fraction of the first fraction, who have assumed such farther modification as

to resist this farther fall. Thus a high body temperature is a sign of a geologically recent and highly specialized form, while a low body temperature is a sign of a primitive form.

There may be a valuable suggestion here. According to it the keeled birds are more recent than the ostrich tribe, while the carnivora and ruminants belong to families more recent and more specialized than that to which man belongs. Man adapts himself to a cold climate by means of his clothes, and by use of his brains. The marsupials seem to have failed to resist the secular refrigeration at a still earlier date, while the vast bulk of the animal kingdom has submitted to the change in its surroundings and become colder and colder blooded, and all life activities slower as time has gone on. The bearing of this upon the relative rapidity of early evolution is obvious. The rate of evolution in the present cold-blooded forms may be vastly slower than in their hotter-blooded ancestors.

But suppose it to be true that there has been a cosmic refrigeration, why should life have waited for its appearance until 44° C. Many forms thrive at higher temperatures, and algæ and low forms occur and thrive in hot springs up to the temperature of pasteurization (75° C.).

If 44° C. be the temperature at which the line of descent of the birds left the ocean and it has fallen since, there seems to have been no reason why it may not have been falling before, carrying all life with it. Indeed, why should it not, if we assume that 75° C. is but barely endurable, while somewhere about 44° is the best for cell life?

The suggestion seems altogether natural and reasonable that both animal and plant life originated at a temperature above 44°, but that they followed the drop of the ocean or cosmic temperature so long as thereby better and more grateful conditions were secured.

We may remark in passing that the drop from 74° to 44° would not be likely to take as long, perhaps not half as long, as the drop from 44° to 14°, for after a body has once fairly started cooling it cools more and more slowly, the lower the temperature.

2. Again, Quinton infers that the early ocean had a concentration of between 7 and 8 parts per thousand of salts. This is the concentration of the blood serum of the birds, which seem to have kept the temperature most nearly constant, and may be supposed to have most nearly the original concentration also? But beyond this analogy this original concentration of about 7 per thousand is supported by a series of striking and important facts which seem to me to form the strongest of all the arguments, and the convincing one, indeed, for a basis of truth for Quinton's law.

The surroundings of fresh-water fishes would tend to lower the concentration. They have indeed somewhat lower concentrations, between 6 and 7 parts per thousand, but not over 8. Salt-water fishes, on the other hand, have a greater concentration. Quinton cites no figure less than 9.3, but they are always less than the present ocean (35). How can we explain the divergence of the two series from a point otherwise than that the fishes, fresh-water and salt, have been derived from ancestors whose concentration was where the one series ends and the other begins, and that the sharks have had their blood grow gradually more saline while the freshwater fishes have suffered a slight dilution of blood. But this is not all. The concentration is in man just about 8 parts per thousand, while in cattle, whose craving for salt is well known, and whose foods naturally lack sodium, so that they are very likely kept a

little short, it sinks to about 7. On the other hand, the dolphin, a relatively recent denizen of the deep, has had his concentration raised to 8.5. There are other facts given by Quinton, regarding fresh and salt-water turtles and crayfish, which I can not give.

But a host of further questions are raised regarding the blood of seals and whales and salmon that breed in fresh-water, and eels that live in fresh water and breed in salt.

We see that this implies that the ocean has grown in concentration. Quinton suggests, as it seems to me rather wildly, that the ocean is losing water into space. Putting this aside, however, we find good reason, in processes now known to go on, to believe that the ocean is accumulating salts and growing more concentrated. The water evaporated from the ocean, carried up into the clouds and rained down again upon the earth, is subject to a natural distillation. It is soft and fresh. On the other hand, the rivers come into the ocean laden with the products of solution. Murray and Dubois have given valuable tables of river composition. The result is that every lake without an outlet, receiving a river, like the Great Salt Lake or the Dead Sea, becomes very salt. And what is the ocean itself but a much vaster lake. So Hunt, Joly, Dubois, Macallum and most of those who have given the subject any especial thought have inferred a concentration of the more soluble salts in the sea. We can not, however, separate a full discussion of concentration from that of the composition of the ocean. I can not see any escape from the general conclusion that there must have been some such concentration, except by supposing that volcanoes from absorbed gases in the interior of the earth are yielding more water than enough to counterbalance the soluble salts brought in by the rivers. This we can hardly disprove, if we assume that there may also be a gradual transudation of these waters from the interior, but it does not seem likely. While, however, Quinton's theory agrees with processes now going on in its suggestion that salts have accumulated in the ocean, we are impelled to ask, as we did in regard to the temperature, why should life have waited to begin until the ocean was already brackish, or why should the ocean have begun with a concentration of 7 parts per thousand?

May it not be that life began in an ocean of much less concentration, but remained open to it, with the body cavity not cut off, until that best for cell life, to wit, a concentration of nearly 7 parts per thousand, was reached?

If we ask this we are led to turn to historical geology, and to the line of enquiry which is, I fear, the most difficult, the least certain, and the most tedious, but to which I feel bound to devote some time, since it happens to be the gateway which led to my interest in the whole subject.

We may ask, what indications are there of any such concentration less than the present in the waters buried in the earlier strata?

Do they come at all, and if so, at the beginning of life, or at that stage in the geological column when the land animals and those who may with some assurance be supposed to have a vital fluid distinct from the sea water are known to have existed? But before we can apply this test we must ask and answer many difficult questions. Can we find analyses of rock waters which can be fairly assumed to represent buried ocean waters? Has not the circulation in strata in the course of millions of years been thorough? Then, again, how can we tell, even supposing that there has been no such circulation that strata were not laid down and filled in the beginning with more

or less fresh water? For freshwater springs, as Hitchcock and others have described them, are not uncommon beneath the ocean, and very commonly fresh water may be found by digging in the sand of ocean beaches.

Such an inquiry would have been impossible in Darwin's time. But since his time so many holes have been put down to very great depths by churn or diamond drill, in search of oil, gas, brine, artesian water, etc., that it does seem fair to ask the question. Yet we can hardly separate in the inquiry the composition from the concentration. For in the proportion of the elements, especially of the chlorine, seems to be the surest test of the character of the water. I shall not try to prove this in detail, as it depends on study of many water analyses, but I may give you some idea of the reason. Chlorine (Cl) now exists in ocean water in excess of sodium, and there is, as we shall see, good reason for believing that that excess has been even greater than now in times past.

By an excess of chlorine I mean that after chlorine has been set aside enough to combine with all the sodium that the analysis of a water shows, there is still chlorine left which is usually understood to be combined with the potassium, calcium and magnesium present.

On the other hand, in waters that are mineralized by leaching, whether from granites of New England or the alkali plains of the west, and in all rivers with very few exceptions, explicable often by contamination with manufacturing wastes, there is sodium enough to combine with Cl and then some left. There is hardly any rock, but the unique Stassfurt deposit from which one can imagine chlorides other than sodium chloride to be leached. Nor, on the other hand, is chlorine in solution easy to get out again. The rare and valuable silver ore, hornsilver, a few significant volcanic chlorides, quite soluble, and transient around active volcanoes, and the deposits of dried up oceans and lakes, and a few minerals of no quantitative importance like apatite and sodalite, complete the list of chlorine minerals.

To get results of any value, one must take all the available analyses of a region, see what the surface waters are, how they seem to be affected in composition as one follows them in depth, eliminate waters that may come from desiccated seas, gypsum beds, or salt beds, or may be erroneous and contaminated. Gradually I find myself forming some idea of what the buried water must have been. I must frankly own that from my studies in that state with which I am best acquainted, Michigan, I was first inclined to think that there was no sign of earlier, weaker ocean waters. On the contrary, deep wells from perhaps every geological horizon seem to have stronger brines than the ocean.

Upon taking a wider view I have come to change my mind, and think it is not altogether accidental that the water at Sheboygan, Wisconsin, has at a depth of 1,340 feet 10 parts per thousand of solids, of which 4.3 are Cl; that the deepest well at Cincinnati, 1,245 feet, has 11 parts per thousand, of which 6 are Cl; that Litton's very careful analysis of the water from a well 2,200 feet deep at St. Louis (the water mainly at 1,515 feet in the Magnesian limestone) should give 8.791 per thousand, of which 4.1 are chlorine, and that near us at Montreal in rocks of similar age in a 1,500-foot well (at 1,190 feet) Adams found 7.57 per thousand, of which 2.46 are chlorine. In Europe the only well at all similar I have yet found is one reported by Struve at St. Petersburg (658 feet through Silurian rocks to the granite) with 3.89 per thousand, of which, however, 2.3 are chlo-

rine. Now compare these with the 35 parts per thousand, of which 20 are chlorine, of the present ocean. Consider that in each case this is the saltest water from the district and horizon. Do they not indicate for the open paleozoic ocean a concentration much less than at present, and not far from that of the vital fluid of the land animals, at the very time the first land animals are known to have appeared? The large collection of water analyses made by Blatchley for Indiana yields exceptionally good material for study, since there are a large number from drilled wells, in many cases separated from the surface by sheets of oil or gas. We have to eliminate only two or three whose extra strength appears to be due to brines of the Salina formation, or to solution of sulphides as sulphates on the one hand, and more numerous shallow wells, where the water is dilute, and carbonates, not chlorides, dominant on the other, to get a well-defined and extra abundant group of analyses, characterized by a chlorine excess, and a concentration of 500 to 1,100 grains per gallon, *i. e.,* 7 to 16 parts per thousand, all drawing their strength deep down in Paleozoic rocks.

Norton's collection[4] gives us a chance to study Iowa in similar fashion. Here there seems to have been much circulation from the high western plains toward the Mississippi Valley. Yet, if the rocks had been laid down in water as salt as the present ocean to any great extent, it seems to me hardly likely that that which contains the most chlorine of all those Norton gives would have but 3.356 parts per thousand, of which 1.1 only are chlorine, while there is a large excess of sodium, showing, to be sure, considerable dilution. This water is from 716 to 845 feet down in the Carboniferous.

This is a work in which it is easy to deceive one's self, and many should share in these critical studies.

[4] Vol. VI., Iowa reports.

3. We come now to the third point made by Quinton, the resemblance of the vital medium to the ocean in composition.

Sodium chloride is the leading salt in both. This is the more noteworthy because in the living cell potassium and phosphates are much more important, and this is also true in most of the animal foods.

That is the reason why the craving for salt is so natural, and the salt tax the one tax that the poorest mortal can not evade, if he would live. It is the last screw to be placed on abject poverty.

A second group of constituents in abundance is formed by magnesium, calcium, potassium and sulphur. Carbon, oxygen, nitrogen, hydrogen (carbon dioxide and ammonia), silica and fluorine are also well known to be present in both. Finally, by an elaborate discussion of physiological litterature Quinton shows that all the rarer elements of sea water, with the exception of cobalt, also circulate in our veins, to wit, I, Br, Mn, Cu, Pb, Pt, Zn, Ag, As, B, Ba, Al, Sr, R, Cs, Au, Li.

I do not consider the identity in the presence of traces of relatively rare elements of any very great importance in the present state of analysis. These elements are all widely distributed in nature, even if in minute quantities, and are liable to occur in ocean or vital medium without there being any genetic connection. Moreover, the knowledge of their presence in the one case or the other depends in a number of cases on only one or two determinations, so that there is no assurance of their universal and constant presence.

When we consider the ratio of the different substances, which is what Quinton means by the composition, we find a number of difficulties as well as some striking resemblances between the vital fluid and

the ocean. One difficulty is in getting what may be fairly called the composition of the vital fluid. Ordinary analyses of the blood, for instance, show percentages of phosphates and an excess of sodium over chlorine entirely foreign to sea water, present or past. But when the blood cells and organic matter have been most carefully eliminated the proportion of phosphoric acid drops to but 25 parts per million, and though this is more than occurs in sea water now, it is very much less than occurs in the body generally, and Quinton's supposition that it is due to a small amount of organic matter that one can not get rid of, or that it comes from excretions from the organic matter which the blood is carrying off, is not unreasonable. We may even, when we look at the analysis of the Sheboygan water, and consider the phosphatic character of the early brachiopods, imagine that there was in early days more phosphorus than the present ocean contains, and that it had been eliminated by life faster than supplied.

Similar explanations might perhaps be given for the excess of sodium.

The ratio of sodium to potassium is in the vital medium 10 to 1. In the ocean it is nearer the famous 16 to 1. This is a great contrast to living matter in general, in which potassium dominates over sodium.

When we come to compare lime and magnesia, however, we find that both in living matter in general and in the ocean at present magnesium dominates (1.31 : 0.47) over calcium. On the other hand, in the vital medium there is three times as much calcium as magnesium. This fact did not escape Quinton and Macallum, and they both suggest the same explanation—that there has been an accumulation of magnesium in the ocean since it determined the composition of the vital medium.

This is chemically and geologically very likely. The magnesium salts are more soluble than the lime salts. Again deposits from the ocean of lime sulphate and lime carbonate with little or no magnesium, gypsum and limestone, are well known. On the other hand, the only common magnesium deposit known in the rocks, dolomite, has a molecule of calcium for every one of magnesium. Thus a relative increase of magnesium over lime seems probable. Dubois brings reasons to believe that the ocean is now saturated with calcium carbonate, and has as much as it will hold, and that lime is thrown out, largely in coral and shell, as fast as the rivers bring in carbon dioxide. The magnesium which the rivers bring will readily remain as sulphate or chloride and is not so easily reduced or precipitated as calcium sulphate, nor is magnesium so freely taken up and thrown out by organic life.

Thus while the problem is not a simple one, since it depends on the supply of sulphur and carbon as well as magnesium and calcium the accumulation of magnesium seems, to say the least, quite possible. In the present ocean Na:Mg:Ca::10.23:1.31:0.47. As regards the sulphur salts, Daly has suggested that, as in the Black Sea so in the early ocean, until the carnivorous habit was well established and a good scavenger system, the sea would tend to be fouled with dead matter and the sulphates brought down by the rivers be reduced and deposited as sulphides, as we find them in black shales and organic limestones.

No doubt this action has occurred from time to time. It agrees with the customary association of pyrite and black shales, and petroleum and sulphur. But what indications are there of greater frequency of sea-deposited sulphides in the pre-Cambrian rocks?

Anyway if the accumulation of magnesium in the ocean depends on the supply of

sulphate we must expect to find it as well as sulphate low in early times.

We are thus brought face to face with a very important problem of historic geology, the chemical evolution of the ocean.

Studying the Paleozoic waters, we find unmistakable indications that they were low in magnesium sulphate, but that they contained relatively large quantities of calcium chloride. This, as long ago noted by Hunt and Goessman, is characteristic of the Paleozoic brines. $CaCl_2$ must have been precipitated little by little by the carbonates and sulphates, forming the calcium carbonate and sulphate of the sedimentary rocks, while the sodium and magnesium remained in solution.

The ratio of calcium to magnesium in the vital fluid is about 0.10 : 0.025, say four to one, while in the Saginaw carboniferous brines it is usually near three to one.

Here for the third time we come to the conclusion that the vital fluid has a composition that the ocean is likely to have had in times past, not at the beginning of its progressive change, but at about the time when we find the first trace of vertebrate life.

4. For the Ottawa meeting of the Geological Society of America I prepared a paper on the chemical evolution of the ocean, and I showed a diagram in which analyses of waters from different strata were arranged according to the ratio of sodium to chlorine, which appeared to have increased in the ocean from something like .20 in Calciferous times to .555 at present. A change in concentration of from 8 to 10 parts up to 35 in the same time would be of the same order, but would imply considerable additions of chlorine in the same time, which we should have to look to the volcanoes and their rocks to furnish. It would also imply, if both changes were uniform, which does not seem likely, that when the concentration by river action began the ratio of sodium to chlorine was somewhat about 0.06. Even in the weakest water and at the beginning the ratio of sodium to chlorine must have had some value, and this is about the ratio in the deep water of the Keweenan copper mines, and may be the ratio which comes from juvenile waters, or those emitted by volcanoes, or may be that due to the leaching of volcanic rocks.

For the fourth time the suggestion is forced upon us that the vital medium does not represent the early ocean, not that which first began to cool, nor that in which the rivers first began to bear their burden of dissolved salts. It does seem quite nearly to correspond to that which we have other reasons to believe existed at the beginning of the Ordovician or end of the Cambrian, not long before the time that the first fishes are known to have existed, but much after life is known to have existed.

The evidence goes then to support Macallum's modification of Quinton's theory (conceived quite independently) that the vital medium represents the ocean water at the time the body cavity in the progress of evolution became osmotically closed.

It thus seems likely that we have found a key by which we may date the development of life and the deposition of beds in terms of the development of the ocean, whenever we can get a sample of the normal ocean water of the time, or approximate to it. I need hardly say that it is not likely that this change in the ocean proceeded at a uniform rate. Changes in climate would affect the great distilling process upon which the development rests. Great uplifts might hasten the supply of salts. Geographic conditions leading to the deposition of enormous salt beds in partially cut-off bays might reduce the con-

centration of the open ocean. As the area of sedimentary rocks increases relatively to that of igneous rocks the character of the supply of salts furnished by the rivers must change.[5] On the whole accumulation was probably more rapid relatively to the area of land surface, at early dates, when evaporation was more powerful. It is doubtful if the land was so well forest clad and protected from erosion as later. Moreover, salts would be less likely to be thrown out when the ocean was weaker and not so saturated.

The broad conclusion of a gradual accumulation of salt in the sea, particularly sodium, is confirmed by so many independent lines of evidence as to have a very small probable error.

Let us review the arguments for accumulation of sodium before leaving this part of the subject.

1. The argument from the erosion of continents developed by Mackie.

2. The argument from the excess of sodium in river water, worked up recently by Dubois after Murray and Joly.

3. The argument from the shortage of sodium in the average sedimentary rock as compared with the average igneous rocks from which it is derived, as developed by Van Hise, Clarke and Mead.

4. The argument which may be drawn from a study of the early buried waters as suggested by Hunt.

5. And finally the argument which as we have seen follows from the composition of the vital fluid and Quinton's law.

These various arguments depend on various lines of facts, so that one may be fallacious without disproving the other. On the other hand, the stronger lend strength to the weaker. For instance, I doubt if any one from mineral water analyses alone would be likely to feel much assurance. The errors and defects in the chemical work, in the collection of samples, and in the circulation and preservation of the waters, are too great.

[5] In early times the supply of chlorine seems to have been relatively more rapid, being derived from the leaching of the Keewatin rocks.

The one thing that seems most assured regarding the buried waters is that the early ones were relatively richer in calcium chloride.

The many published, and dozens of yet unpublished, determinations upon our Michigan waters, have put that in my mind beyond question.

The analyses of the coastal plain post-Paleozoic waters deserve farther study. The saltest well of the analyses recently collected by Smith in Alabama from the Tertiary has 30.5 parts per thousand with 11.47 sodium against 18.52 Cl., while the ratio of magnesium to chlorine is .1224: .247.

This is not far from what we might expect from a buried sea water of this age, but on the other hand among the waters from the Cretaceous there is none that appears to be at all unmixed sea water. The saltest, and at the same time one of the deepest (IVa) has only 8.57 per thousand and Na:Cl::2.999:4.538 = 0.66, while Mg: Ca::.0431:.1396.

The analyses collected by Veatch in Louisiana, where we know that rock salt beds occur, show in many cases greater concentration of salt than the present ocean. They also show very little of sulphates, but they show a ratio of magnesium to calcium quite different from the Saginaw brines which mark them as relatively recent.

Leaving now the tedious numerical part of our subject, we may ask ourselves what bearing this has upon the difficulty to which we referred at the beginning.

The vital fluid seems to date or preserve the ocean composition, concentration and

temperature of somewhere about Beekmantown eo-Ordovician time, somewhere about the time that the ocean had attained one fifth of its present concentration. This indicates that the pre-Paleozoic was probably not one fourth as long as later time, dating the beginning when the ocean began to get concentrated.

Thus the rapidity of early evolution, to which physical, astronomical and stratigraphic evidence had led us, is confirmed.

But we know that life existed all through Cambrian time, and earlier yet. So while Quinton's investigations indicate certain conditions of the vital circulating fluid from which fresh-water and salt-water vertebrates have diverged, while the more vigorous land animals have retained them nearly unchanged, these conditions are by no means so near the beginning as one might think from a casual reading of his argument. It seems much more probable that life began as soon as was possible, at higher temperatures than 44° C. and at very low concentrations. The water was that leached from basic rocks, the Keewatin schists, and was relatively richer in calcium chloride, and I suspect also ferrous chloride,[6] and was in composition as well as concentration by no means the best for organic life. We may then believe that as the temperature of the water decreased, and the concentration increased, this change was in the direction of the physiological optimum, or salt solution most favorable to protoplasm and cell activity.

According to Meyer's account of the stimulant effect of sodium chloride, calcium and potassium, and the sedative effect of magnesium, the early ocean, as it accumulated salts of sodium and lime, must have been a more and more stimulating medium, up to a point when it became overstimulating and poisonous. Up to this time, which I take to be about the beginning of the Cambrian, there would have been no physiological tendency to secretion or excretion of lime by animals, until and unless the ocean became supersaturated with carbonate. The ocean in the beginning must, like fresh water to-day, have acted as an active solvent if any were formed. But when it passed the optimum then the excretion or precipitation of lime and accumulation of magnesia, which is more sedative, might tend to restore the balance. So long as it could thus be kept in most favorable condition for cell life, there would be no especial reason for osmotic closing or the development of a special vital fluid. Up to this point it would pay animals to accept the beneficial changes which were taking place in the medium in which they lived and moved and had their being.

We do not, understand, suppose that the ocean arrived at the best conditions for cell life in all respects at the same time, but we suggest that one of the first endeavors of the more vigorous animals to keep the vital medium of the best was by means of secretion of superfluous lime. This is supported by the fact that lime is the substance most abundantly brought in by rivers, one in which saturation would soon be reached of the carbonate, and by the fact that lime skeletons and hard parts begin to be abundant in the Cambrian, a geologic period before those animals appeared which we may be sure had a separate vital fluid. However, our general belief in the critical importance in the history of life of the time when the ocean passed through the most favorable conditions for cell life does not depend on any particular theory of the physiological interrelation of various salts.[7]

[6] Compare the richness of iron carbonates in the Huronian. Probably mixed carbonates were precipitated as fast as carbon dioxid was furnished.

[7] But we do suggest that the secretion of hard parts began first as a physiological reaction, like renal calculi, to the increasing hardness of the vital medium.

As the ocean passed more and more decidedly this optimum, the more vigorous organisms resisted this change in various ways. They cut themselves off from it osmotically. They secreted a more or less impervious carapace or shell. Then they got out of it entirely and migrated to the air or the land, perhaps by way of the shore sands and muds. This period when the ocean seems to have passed its best stage for life appears to have been the Cambrian. After this period there was a wealth of forms able to leave hard traces of themselves. Before this period there was no physiological need for skin or shell. But once the skin and shell had been developed, primarily as a physiological reaction against the water, their great advantages for purposes of defense and support no doubt soon made themselves felt.

Before this the early dilute and perhaps acid ocean water would attack shells freely. After the Cambrian time there was an excess of calcium carbonate, which has been steadily thrown out, as the rivers brought the carbon dioxide in, ever since.

In passing I may say that Macallum has suggested that from the composition of the protoplasm itself we may form some idea of that of the early ocean, but as I can not endorse his conclusions, I will not dwell on them here.

We thus attribute the development of hard parts and a separate vital medium, the one occurring at the beginning, and the other at the end of the Cambrian, to the same cause, the endeavor of the societies of cells we call organisms to maintain for the mass of the constituent cells the best possible conditions for their activity.

We have thus, I conceive a fair explanation for the rarity of traces of hard parts in the early rocks. The animals had felt no physiological need of them and had not begun to develop them, and the ocean was relatively fresh, and would more easily dissolve them.

Moreover if the early ocean was changing in composition and growing more favorable to cell life, while the organisms were bathed in it, we might well expect a rapid evolution, and one not merely superficial.

It is a standard doctrine of biology that the individal in his growth gives a sketch of the history of the race to which he belongs, that embryology and phylogeny run parallel. I do not know that any one has heretofore suggested that the time occupied in passing from one stage to the corresponding stage is in any way proportional in the two series, and that the rapid changes in the egg at the beginning are matched in racial history. It would be indeed foolish to hold this in any rigid way, for the parallelism is in no sense strict. Yet is it not fair to suppose that life as a whole was more plastic at the start even as the individual is, and that as in artificial races there is a cumulative effect of heredity, tending to hold them true after a few generations, so it must be for organic life as a whole?

As we have said, if we may estimate by the concentration of the vital medium relative to that of the present ocean, and suppose the increase to have been uniform, the time prior to the separation of a distinct body fluid, that is, the pre-Ordovician, can be only a quarter of the time which has elapsed since. A discussion of the temperatures and change of ratios of sodium to chlorine and magnesium to calcium, in the present ocean and Paleozoic waters while they do not agree closely, also lead to the conclusion that the ocean at the beginning of concentration by erosion was only a fourth older than the early Paleozoic ocean.

This will give us room for all the sedi-

mentary column of Huronian and Laurentian, I think, for I think the pre-Ordovician column of rocks is hardly more than one fourth of that since.

I presume some will grumble at being curtailed to so short a time before the Trenton, only twenty million years or so (if the time since is eighty million years). As we have said, the progress is probably not exactly uniform. But really twenty million years is quite a while. The following illustration may help us to appreciate this. There has been a change of 15 per cent. to 20 per cent. in the flora of Michigan and Ohio in the past 200 years. If then in each year there was an average deposit of but one two-hundredth of a foot there could be an accumulation of ten thousand feet of strata with a 15 to 20 per cent. change of plants every foot in twenty million years. Have we any facts to make us feel sure that that is not time enough?

The growth of scientific doctrine and theory is like that of some modern invention like the steam engine. Different men contribute, the one this improvement, and one that, until in looking at the perfect machine one wonders and admires and forgets that it is an embodiment of the ideas not merely of one designer, Watt, or Corliss, or Nordberg, but of many, who have each contributed something. So many have discussed the physiological salt solution and the oceanic origin of life, but Bunge, Macallum, and especially Quinton have brought them into relation. Before them Hunt, Goessman, Joly, Mackie, Dubois and others have theorized on the evolution of the composition of the ocean.

From them all we have borrowed, or by them been anticipated. So of previous writers on the conditions of early life, it will be seen that I agree with Chamberlin as to the relatively fresh character of the early medium in which animals appear, that I adopt Daly's suggestion that the scarcity of hard parts of pre-Cambrian animals was physiological and due to the chemical character of the ocean, though we can not at all agree with his conclusion that it was limeless, which seems to be negatived by the composition of the vital fluid, the evidence of fossil brines, and the deposits of the early oceans.

What we bring (besides some detail studies of buried waters) is the correlation, and the suggestion that the development of hard parts, and a relatively permanent vital fluid, were *both* physiological reactions to the chemical evolution of the ocean, as it reached and passed its best conditions for life in the early Paleozoic.

The wider the area of our knowledge the greater the circumference of our ignorance, and the test of a good theory is that it opens up new lines of research. Let me mention a few. Further tests of the composition of the vital medium would be very interesting, especially in salmon, and eels and the like, as well as in seals, whales and insects.

The field which I hope to help cultivate, myself, is the study of waters which may be in part buried sea waters. It would be very interesting to extract the quarry moisture of impervious rocks. I do not know how to do it, without danger of extracting solid constituents at the same time.

To sum up in conclusion, it seems likely that early evolution was very rapid—the history of the race in this respect being like that of the individual—because of some of the following factors:

1. A warmer ocean, and consequent greater activity of life.

2. A constant approach of the same up to early Paleozoic times toward better conditions for life, which caused the organisms not to cut themselves off from it, but remain open, while hard parts were rare, thus

exposing the organisms throughout to the modifying effects of environment.

3. The relative weakness of the stereotyping effects of cumulative heredity at this early date.

4. The fact that as all available spots were not preempted, there were wide fields open to successfully modified forms adapted to some new yet unoccupied station, who could then be very prolific, and thus give large play for further adaptation.

5. The frequency of generations in the lower animals and plants.

6. Probably a relative lack of seasonal rhythm.

7. While new forms of life and the flesh-eating habit were being developed a stimulus was put on various modifications to meet these new conditions.

Since early Paleozoic times animals have existed fitted for land and sea, salt water and fresh, air and mud, herbivorous and carnivorous, with the main methods of attack and defense outlined. So that one could hardly expect so radical or rapid changes thereafter.

I think this audience in this assembly will permit an old pupil of Shaler to indulge in a little philosophy and close on the eve of Sunday with a moral.

Haec fabula docet: that those societies of cells known as animals have not been the mere slaves of environment, nor even of environment and heredity conjointly, but have struggled, with more or less success, to maintain through varying environment that part only of their heredity which conduced to greater protoplasmic activity (or, to put it in every-day English, have striven to surround the great mass of the cells of which they are made up with the conditions best for their health and vigor), and the physical grade of the animal is in the ratio of its success in this struggle for the common weal of the constituent cells.

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A. C. LANE

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SCIENTIFIC BOOKS

Researches on the Affinities of the Elements. By GEOFFREY MARTIN.

The unfortunate part of the book is that any attempt to separate the grain from the chaff is made difficult by the amount of chaff.